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Interactive comment on "Ocean acidification decreases plankton respiration: evidence from a mesocosm experiment" *by* K. Spilling et al.

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The authors report on some results of a whole ecosystem CO2 enrichment study using large volume mesocosms moored in the Gulf of Finland as part of a special issue on "Effects of rising CO2 on a Baltic Sea plankton community: ecological and biogeochemical impacts". This report makes a substantial contribution to the issue by focusing on the important processes of carbon exchange through primary production and respiration. Overall the study is complex and multidimensional, and it would have been difficult to review this article outside the context of a special issue as many important details of the experimental design and basic observations on the mesocosms appear in other reports. Fortunately, with the open discussion format of Biogeosciences Discussions, these other reports were accessible to the reviewer. A key result is that respiration decreased as a function of CO2 enrichment though the difference only emerge

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towards the end of the experiment when composition of the mesocosms (even controls) had substantially diverged from surrounding waters. But the question is left open as to what happened to the extra carbon, as the study did not observe a concomitant increase in net primary production.

While the results do demonstrate decreased respiration for samples from the higher CO2 enrichments, I do have some concern about how representative these rates are of processes in the mesocosms. A depth integrated water sample was taken and incubated at "ambient" temperature. But it can be seen from Paul et al (2015) that there was a strong temperature gradient over the mesocosm's depth range, at times as much as 10°C, so it is not clear what was "ambient" temperature. Moreover, mixing waters of differing temperatures may bias the respiration measurement at a fixed temperature vs. the "real" average, i.e. combining warm, lower particle concentration surface water with cooler, high particle (or nutrient) concentration bottom water could stimulate respiration versus the average of the two.

The authors also indicate that respired carbon was about 10x greater than net production (pg. 17 line 7). Some more explanation is needed for why such comparison is made since a determination of whether the system is net heterotrophic or autotrophic would require comparison of gross primary production with total community respiration, as stated on page 21 line 9. The statement on page 21 line 26 implies that the authors have some idea of gross primary production, could this be compared to respiration rate?

The authors also speculate that the net primary productivity method may not have been sensitive enough to detect difference between treatments, so that enhanced production at increased CO2 was not detected. Small incubation volumes are suggested to contribute to uncertainty but the authors give no indication of what was that measurement uncertainty. Nevertheless, they state that the measurements were comparable to previous ones in the same regions using similar methods (Kivi et al. 1993) which would argue against any substantial bias. One other factor to consider as to whether

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the NPP assay would detect an enhancement effect was that the incubations were conducted outside the bags. According to Riebesell et al. (2013), the mesocosm material (thermoplastic polyurethane) removes all UV whereas glass scintillation vials used for the NPP incubation transmit UV-A and most UV-B so rates in the vials could have been substantially more inhibited in the near surface samples than phytoplankton in the mesocosms that were protected from UV. Moreover, some studies have shown that phytoplankton grown under CO2 enhanced conditions are more sensitive to UV. It is possible that NPP was higher in the mesocosms with CO2 enrichment but the effect was dampened in incubations outside the bag due to a counterbalancing increase in sensitivity to UV (see, e.g., Sobrino et al. 2008, 2009). Also, as the lead author knows (since he was co-author on the paper), Sobrino et al. (2014) observed lower rates of DOC release during short term PPR incubations by phytoplankton acclimated to CO2 enhanced conditions but this effect was much less when incubations included UV. This DOC would be quite labile and rapidly respired so might not affect the bulk DOC pool but a reduction in DOC release could decrease bacterial respiration.

Specific Comments:

The lack of UV in the bags should be mentioned in the text, e.g. :

Pg. 20 line 5 " light and temperature were similar both inside and outside the mesocosm bags. "

Except that UV was absent inside the mesocosms.

Page 22 – The discussion finishes abruptly, a summary paragraph would be helpful

Respectfully submitted,

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